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RECORD OF OBSTETRICAL CASES.

[Communicated for the Boston Medical and Surgical Journal.]

AT a meeting of the Middlesex East District Medical Society, held in January, 1855, it was proposed that some blanks, for the record of such obstetrical cases as should occur in the practice of members, should be furnished, and that when a sufficient number should be filled, they be published.

There are five hundred and eighty-six cases recorded by eight gentlemen, in the years 1855 and 1856. If the average number per year should hold good throughout the profession in this State, viz., 37, it is readily seen how vast a number of observations could be collected, at very little expense of time and trouble to each individual.

It is not pretended that the form of the blanks is perfect, nor that there may not be some imperfections in the records; but, from the well-known character of the practitioners who have so kindly responded to the appeal, the compiler can promise as great a degree of accuracy, *as far as the records go*, as may be found elsewhere.

Much can be and is said concerning statistical information. The subject has been treated by others far better than I could do it, and therefore I will not attempt the theme, but proceed, at once, with the tables, the summing up of which has involved no small degree of time and labor.

There were 586 births, and 596 children born; 10 twin births, being 1 in $58\frac{6}{10}$ cases; 300 males, 296 females.

Not reckoning one case of an American, reported as having had pains four weeks, the tables give as the average duration of pains among

	H. M.		H. M.
383 American women,	14.00	5 Canadian women,	19.25
175 Irish "	15.20	3 Scotch "	11.15
6 English "	7.45	1 P. E. Island,	3.00
6 Nova Scotian "	15.09	1 French	2.15
5 German "	15.12		

The longest time of these was 6 days; the shortest, 15 minutes.

208	were 1st births.	17	were 7th births.
125	" 2d "	14	" 8th "
83	" 3d "	8	" 9th "
60	" 4th "	5	" 10th "
47	" 5th "	1	" 11th "
17	" 6th "	1	" 12th "

No. of children born in February,	26	No. of children born in March,	53
" " November,	39	" " April,	53
" " August,	42	" " October,	53
" " January,	48	" " September,	53
" " July,	50	" " June,	54
" " May,	52	" " December,	63

From 6, P. M. to 12, A. M., there were, births	149
" 12, M. to 6, A. M., " "	149
" 6, A. M. to 12, M., " "	150
" 12, M. to 6, P. M., " "	138

In 558 observations the average time of the "breaking of the waters" previous to birth, was three hours and fifteen minutes. Longest time was three weeks; many cases at birth.

The proportion of miscarriages that had befallen 384 American women, previous to these records, is 1 in $4\frac{1}{6}$; 175 Irish, 1 in $6\frac{1}{2}$; 1 to 6, English; 1 to 3, Nova Scotian; 5 to 5, Canadian; 2 to 1, Prince Edward's Island; 1 to 1, French; 1 to 1, French Canadian.

In 574 single cases there were 17 other than vertex presentations, one in about 34 ($33\frac{13}{17}$), viz. : face to pubes, 1 in 82; breech, 1 in $95\frac{1}{2}$; footling, 1 in 287; shoulder, 1 in 287. Average time in attendance, $4\frac{1}{2}$ hours.

Twin Cases.

First. Presentation of first child, natural; of second, feet. Both males: first weighed 5 pounds, lived; second died, "apparently a three months' child."

Second. Presentation of first, natural; of second, breech. Both males—died.

Third. Presentation of first, face to pubes; of second, footling. Male and female.

Fourth. ? Male and female.

Fifth. Presentation of first, natural; of second, feet and nates. First gasped a few times. Second, double hare-lip; fissure of hard and soft palate; seven months' child; cried; cerulean; moaning; died.

Sixth. Presentation of first, breech; of second, natural. Females; one died in 4 weeks, of old age.

Seventh. Presentation of first, natural; of second, breech. Males; one died in six days, of a meddlesome nurse.

Eighth. Presentation of first, natural; of second, feet. Females; nine previous labors; both alive now, at fifteen months.

Ninth. Presentation of first, natural; of second, feet. Females; one died on the third day, from gas, being placed too near a coal stove.

Tenth. Presentation of first, natural; of second, feet. Females; seventeen hours between the two; first was with difficulty got to breathe; lived some ten or twelve hours. Second born putrid.

There is some "experience" in our District, which may be placed by the side of that occasionally gained in the metropolis; for instance:

"A long case; took ol. ricini for costiveness; tart. ant. for closed os; ergot for slackened pains; the vectis and chloroform at the finale. Sick in a room with the thermometer below 0; patient under two or three comforters. Delivered her with dreadnought overcoat on; mother and child did well." And so did the doctor.

"Mother did poorly; opened an abscess in left labium; afterward had mammary abscess and severe pleuritic inflammation. Phthisis became developed, and she died in four months."

The following seems simple; it is much in little:—"Found arm presenting, shoulder crowded down into pelvis; woman healthy, strong, muscular; turned and delivered; mother did well."

"Delivered with forceps; mother did well. Convulsions came on after delivery, apparently from the enormous collection of coagulum in the uterus, which was broken up by the hand, and convulsions ceased."

I will not ask for more space in your Journal at this time, than I have already taken up. I think I may promise, however, that, at some future time, referring to these tables as an earnest of a sincere desire to obtain and impart knowledge concerning the obstetric branch of our profession, I may ask for a page or two, in which the present paper may be extended.

For the Middlesex East District Med. Society,

WILLIAM INGALLS,
Secretary.

Winchester, April, 1857.

DR. EDWARD BROWN-SEQUARD'S EXPERIMENTAL AND CLINICAL RESEARCHES APPLIED TO PHYSIOLOGY AND PATHOLOGY.

[Concluded from page 220.]

THEORIES of epilepsy, entirely at variance with the preceding, have been proposed. In the last century, Saillant (*Expér. sur des Animaux pour découvrir le siège et la cause prochaine de l'épilepsie*, in *Hist. de la Soc. Royale de Médecine*, in 1782 and 1783, p. 88–96), without giving a theory of epilepsy, concluded, from some experiments, that it is easier to cause epileptic seizures in producing alterations in the blood than by irritating the nerves or the brain. Had galvanism been known at the time of the researches of Saillant, and had he employed it to irritate the nervous centres, he would have seen much more violent and lasting convulsions than

those he observed after having altered the blood by injections of air, &c. His experiments only show that convulsions may be due to altered blood, a fact well known already before his researches.

One of the most eminent medical writers of our times, Dr. R. B. Todd, has recently proposed a theory of epilepsy, which I must discuss at length, on account of the importance it should have if it were true, and of the value that belongs, necessarily, to any opinion held by such an ingenious and experienced physician.

Dr. Todd says, "I hold that the peculiar features of an epileptic seizure are due to the gradual accumulation of a morbid material in the blood, until it reaches such an amount that it operates upon the brain in, as it were, an explosive manner; in other words, the influence of this morbid matter, when in sufficient quantity, excites a highly polarized state of the brain, or of certain parts of it, and these discharge their nervous power upon certain other parts of the cerebro-spinal centre, in such a way as to give rise to the phenomena of the fit."

Dr. Todd then proceeds to say that a very analogous effect is observed when strychnine is given to a cold-blooded animal. This drug may be administered in very minute quantities for some time without producing any sensible effect; but when the quantity has accumulated in the system up to a certain point, then the smallest increase of the dose will immediately give rise to the so-well-known peculiar convulsive phenomena, observed in this kind of poisoning. Dr. Todd adds: "This, then, is the humoral theory of epilepsy. It assumes that the essential derangement of health consists in the generation of a morbid matter, which affects the blood, and it supposes that this morbid matter has a special affinity for the brain or for certain parts of it, as the strychnine, in the case just cited, exercises a special affinity for the spinal cord. The source of this morbid matter is probably in the nervous system, it may be in the brain itself. It may owe its origin to a disturbed nutrition—an imperfect secondary assimilation of that organ—and in its turn it will create additional disturbances in the functions and the nutrition of the brain."

"According to the humoral theory, the variety in the nature and severity of the fits depends on the quantity of the poisonous or morbid material, and on the part of the brain which it chiefly or primarily affects. If it affect primarily the hemispheres, and spend itself, as it were, on them alone, you have only the epileptic vertigo. If it affect primarily the region of the quadrigeminal bodies, or if the affection of the hemispheres extend to that region, then you have the epileptic fit fully developed."—(*Medical Times and Gazette*, Aug. 5, 1854, p. 129.)

This theory is nothing but an ingenious hypothesis which Dr. Todd proposes, without trying to prove it. The only reason he adduces to support his theory is, that in the renal epilepsy there

is very likely a poison in the blood, but as regards the other kinds of this convulsive affection, he does not say any thing which may lead to the admittance of his hypothesis. Feeling that he had no proof of the correctness of his views, he says: "To give a more definite character to the humoral theory, we need to discover a morbid matter in the blood, in variable proportions, in every case of epilepsy. This desideratum has, as yet, been only partially obtained." Dr. Todd alludes here to the influence of the accumulation of urea in blood, in the cases of renal epilepsy. Leaving aside, for a moment, this kind of epilepsy, we may say against the humoral theory of the eminent British physician: 1st, That we do not know any fact in favor of it; 2d, That there are a great many facts in opposition to it.

Not only we do not know any fact favorable to this theory, but its author seems to be like ourself, in this respect, as he does not relate a single one. We have never read or heard that a poison produced in the brain, has been found in the blood of epileptics, and we cannot imagine on what ground a fact of this kind is considered as probable by the author of the humoral theory or rather hypothesis.

To establish the humoral hypothesis on a solid basis, it would be necessary to show: 1st, That there is always a poison in the blood of all epileptics; 2d, That this poison gradually accumulates in the blood until its quantity has become considerable enough to produce the phenomena of the fit; 3d, That during or after a fit, this quantity diminishes (because if it were not so, the fit would continue or come again and again, after a very short time); 4th, That the nature of the poison varies, so that it acts either on the brain proper alone (producing a mere vertigo), or on the other parts of the cerebro-spinal centre alone, or on the whole of this centre at once; 5th, That this poison has quite a different influence on the brain proper and on the other parts of the cerebro-spinal centre, destroying the actions of the former and increasing excessively the actions of the latter.

Not only none of these points have been made out, but it seems that no attempt has been made in the way of a demonstration in this respect.

That there is a poison in the blood of epileptics is a fact which, nevertheless, is possible, as there are substances in the blood of every man, healthy or epileptic, which by a transformation or by accumulation, may act as poisons, and be the cause of many of the phenomena of an epileptic seizure; but it is not known whether the quantity or quality of these substances is changed in epileptics, just before the fits.

There are many facts which are in direct opposition with the humoral theory of epilepsy. Certainly it is so for all the cases in

which a ligature around a limb or one of its parts, has prevented a fit, and also for the cases in which epilepsy has been cured by the section of a nerve, by an amputation, by the extirpation of a tumor, a tooth or a foreign body, or by the expulsion of calculi, of worms,* &c. If, in all these cases, there was, as the cause of the phenomena of the seizure, a peculiar influence of some poisonous matter on the encephalon, instead of an irritation springing from certain peripheric nerves, the means mentioned would not prevent the fits, and, still less, effect a complete cure of the disease. If we were to admit that it is a poison which causes the phenomena of the seizure in these cases, we should have to admit also that this poison acts on the peripheric parts of some nerves, and not on the encephalon. But there is no more ground for this last hypothesis than for the preceding, because the presence of a poison in the blood is a mere supposition. Besides, if this would be a reality instead of a gratuitous supposition, it would remain to be explained why this poison does not act in some way or other after the section of a nerve, or the extirpation of a tooth, &c.

The humoral theory is in opposition with many other facts, among which are those proving that an emotion or various other moral causes may produce a fit of epilepsy. For cases showing, without any doubt, the influence of these causes and their relative frequency, I will refer to the works of Delasiauve (*Loc. cit.*, p. 219–22) and Moreau, de Tours (*De l'étiol. de l'épil. et des indications*, &c., in *Mém. de l'Acad. Impér. de Médecine.* 1854. Vol. XVIII. p. 1 *et seq.*)

The facts we have related in § XI., to prove that seizures of epilepsy are sometimes produced by a mere pressure upon or by galvanization of a small part of the skin, are also in direct opposition to the humoral theory. How could a pressure upon the skin produce a fit, every time it is made, if the fits were due only to a peculiar influence of a poison on the encephalon?

The following facts resemble, in many respects, those I have mentioned in § XI., and they also are in complete opposition to the humoral theory: they have been collected by Delasiauve (*loco cit.*, p. 137–38), to show the influence of certain circumstances on some epileptics: 1. A patient could not smell hemp, without having a fit.—(*Tissot.*) 2. In another, the same effect was produced by the slightest odor, even that of broth or of a medicine.—(*Schubart.*) 3. A child had a fit every time he saw something red.—(*Buchner, Tissot.*) 4. A child had an epileptic seizure as

* A curious case of hystero-epilepsy, due to larvæ in the frontal sinuses, has been recently published by Messrs. Duménil and Legrand Dussaule. These larvæ, which belonged to five different species, were expelled by the nose, and after their expulsion the patient, who had had violent convulsions for many months, was cured. (See the very useful report on the progress of medicine and surgery, entitled *Annuaire des Sciences Médicales*, par le Dr. Lorain, revu par le Dr. Ch. Robin. Paris. 1856. p. 151.)

often as he heard a dog bark.—*(Van Swiéten.)* 5. The idea of phantoms, which had frightened a boy, when quite young, was sufficient to cause the fits.—*(Maisonneuve.)* 6. In a case, the remembrance of a fright was enough to produce the seizure.—*(Van Swiéten.)* 7. Any word of blame, addressed to two patients, gave them a fit.—*(Delasiauve.)*

The cases in which a physical impression has been the cause of the first attack of epilepsy, may be regarded as less valuable against the humoral hypothesis, than the preceding facts in which, at each return of the cause (either moral or physical), a seizure took place. It might be said to diminish their value that the physical impression occurred just at the time when the poison of the blood was beginning to act upon the brain. But in admitting that such a coincidence has sometimes taken place, we certainly cannot imagine that *in all the very numerous cases of epilepsy*, in which the first fit has occurred immediately after a physical impression, such a coincidence has existed. The works of the principal writers on epilepsy, Van Swiéten, Tissot, Maisonneuve, Cooke, Esquirol, Portal, Copland, Herpin, Delasiauve, Moreau (de Tours), &c., contain too many of such facts for our dreaming of the possibility of explaining the production of epileptic fits, immediately after a physical impression, without attributing at least a share in the causation of these fits, to this impression. The *post hoc, propter hoc*, is a sound reasoning when the number of facts is so extremely considerable as it is here.

In my animals, as I have already said many times, the fits are produced at every time the skin of certain parts of the neck and face is pinched.* As the seizure in these animals takes place when we desire it, we have there a decided proof that, at least in them, fits may be produced otherwise than by the irritation of a poison on the encephalon.

It results from this exposition of facts that, in animals and in man, fits of epilepsy cannot be considered as always due to the influence of a poisonous matter upon the encephalon. We would not say, however, that they are never caused by a poison in the blood. It seems, on the contrary, not only when there is a deficiency in the urinary secretion, but also when the elements of bile are in great quantity in the blood, or when the functions of the supra-renal capsules are suppressed, that epileptiform seizures take place, owing to the irritation that certain substances, contained in the blood, exert upon some parts of the nervous system. When there is not a free menstruation, and perhaps, also, when the secretion of the skin is stopped, it seems probable that a poisonous matter remains in the blood, where it accumulates, and that it par-

* In October, 1855, I had the satisfaction of showing this experiment to Dr. R. B. Todd himself, in presence of many distinguished physicians, among whom were W. Bowman, Prof. L. Beale, Dr. R. H. Semple and Dr. R. Druitt.

ticipates in the causation of epileptic fits.* Besides, it is certain that some poisons, and particularly lead, are able to cause epilepsy. But many questions are still to be solved, concerning the *modus operandi* of poisons which cause convulsions. I have shown elsewhere (*Experimental Researches applied to Physiol. and Pathol.* New York, 1853, p. 57–63 and p. 113) that these poisons have two modes of action, entirely different one from the other. One of these modes, which is by far the most frequent, seems to consist only in an increase of the reflex faculty of the cerebro-spinal centre. The poisons which belong to this category, according to my researches, are the following: strychnine, brucine, cyanhydric acid, cyanide of mercury, morphine, nicotine, picrotoxine, digitaline, sulphide of carbon, oxalic acid, &c. The other mode of action of poisons producing convulsions, consists mostly in a direct irritation of various parts of the nervous system. I do not know of any other poison, acting exclusively in this way, except a substance existing normally in the blood, which accumulates during asphyxia, and which very likely is carbonic acid. The differences between these two modes of action of poisons are striking. In one of these modes there is no irritation, or at least very little, produced upon the nervous system or the contractile tissues, and therefore there is no convulsion directly caused by the poisons belonging to this category.

It will probably surprise many persons to hear that strychnine, cyanhydric acid, brucine, &c., do not directly give convulsions—but this is a fact; these substances do not seem to have any power of excitation either on muscles, on sensitive and motor nerves, or even on the spinal cord. Perhaps some of the poisons, of which a list is to be found above, have a slight power of excitation on the spinal cord, but they certainly do not cause directly the powerful convulsions which are attributed to them. They act almost only in increasing the reflex power of the cerebro-spinal centre, in such a manner that the least excitation, as, for instance, a voluntary or a respiratory movement, or any other kind of irritation of nerves of the skin or of the mucous membranes, causes convulsive reflex movements. We might say that they act in giving to the nervous centres the faculty of causing convulsions *when the centres are irritated, but they do not irritate.* (For the proofs of these views, see my work above quoted, p. 57–63.) On the contrary, black blood, or very likely carbonic acid, seems to destroy the reflex power of the cerebro-spinal centre, but at the same time it *irritates* violently this centre, and, therefore, causes directly powerful convulsions. This last poison differs also from the preceding in being able to irritate directly muscles and motor or sensi-

* Very judicious remarks on the subject of the influence of poisonous matter contained in blood, in eruptive diseases, in jaundice, in deranged menstruation, in albuminuria, &c., have been made by Prof. Gunning S. Bedford, in his important work, *Clinical Lectures on the Diseases of Women and Children.* Third Ed. 1856. pp. 437, 475, 502 and 525–34.

tive nerves. (See for this and other influences of black blood, or rather of carbonic acid, my work, already quoted, p. 110–13, and p. 117–24. See, also, the thesis of my friend and pupil, Dr. Brandt, entitled *Des phénomènes de contraction observés chez des individus morts du choléra ou de la fièvre jaune,* Paris, 1855, and my paper on red and black blood in the *London Medical Times and Gazette,* Nov. 17, 1855, p. 492–94.)

There are, therefore, some poisons that cause convulsions indirectly, by increasing the reflex power of the cerebro-spinal centre, and not in irritating them, while there are others which cause convulsions directly by an irritation of the cerebro-spinal centre. In which of these two categories are we to place the poisons, contained in blood in cases of epilepsy, where some secretion (the urinary, the biliary, &c.) is suppressed or much diminished? This is quite an undecided question. Many other things are still to be known concerning these poisons; but we do not intend to examine this subject here. We wished merely to say, that even in cases where there is some ground for the humoral theory of epilepsy, proposed by Dr. Todd, we have no proof that the poison acts as this eminent physician supposes. We will add that even in cases of organic disease of the kidney, coincident with epilepsy, we are not entitled to declare positively that it is in consequence of the accumulation of some of the principles of urine in the blood, that the fits are produced, as it might be that they result from an irritation of the renal nerves, as it is the case when there are calculi in the tubuli of the kidneys without a notable diminution of the secretion of these glands. On another side it is very well known, as Prévost and Dumas, Ségalas, Tiedemann and Gendrin, Mitscherlich, Bernard and Barreswil, Stannais, Frerichs and myself have ascertained many times, that after the extirpation of the kidneys, *i. e.,* when the urinary secretion is as much diminished as possible, convulsions are very rarely produced, and never violent. So that in a case of epilepsy with renal disease, either the convulsions have no relation whatever with the renal affection, or if they have a relation, it is either through the agency of the renal nerves, or in consequence of a transformation of some element of the urine in the blood, as these elements seem to be unable to cause convulsions. It is mostly this last argument which has led Frerichs, in his very interesting work on Bright's disease (*Die Bright'sche Nervenkrankheiten,* Leipzig, 1852), to his so-much-debated theory of uræmia.

As a general conclusion of our discussion of the humoral theory of epilepsy, we will say: 1st, that even in the cases where there is probably a poison in the blood, its relations with the production of fits is not known. 2d, that we are not entitled to consider as due to the elements of certain secretions, remaining in the blood,

the epileptic fits which may exist when the glands producing these secretions are diseased. 3d, that there are a great many cases of epilepsy in which the cause of the fit is not in the blood.

THE NEW PANACEA—DR. COGGSWELL'S ANTIPHLOGISTIC SALT.

[Communicated for the Boston Medical and Surgical Journal.]

"WHY not try it? If you don't know what it is, why don't you try it in a case which it can't injure, and test its properties? Dr. Coggswell is not one of the uneducated or self-taught quacks, but a physician. He has the degree of Doctor of Medicine. He has spent years in investigating disease, and he *may* have made a discovery which is all that it pretends to be." Such are the remarks that every physician is called upon to hear and reply to, whenever a new quack medicine is advertised. Commonly, one does not care to make any sort of answer, but the time comes when feelings of particular regard for a suffering friend, lead him to take more than ordinary pains to prove the absurdity of trying every new advertised nostrum, and the deceptive powers of its proprietor.

The Antiphlogistic Salt has been advertised very largely, and the advertisement paid for in the medicine itself. So say the papers. After a month or two of gestation, the child has been brought into the world. Now for the after-birth. The *Traveller*, *Transcript*, and I know not what other papers, have published extracts from twenty-seven newspapers, giving accounts of the marvellous effects in headache, neuralgia, ague, palpitation of the heart, rheumatism, bronchitis, croup, erysipelas, fistula, scrofulous carbuncle (?), catarrh, canker, salt rheum, ulcers, dyspepsia, fits, pleurisy, heartburn, fever and ague, inflammation of the lungs, nervous and female complaints. There's a list for you, and the *Traveller* precedes it with this announcement:

"EDITORS ON DR. COGGSWELL'S SALT.—We think the following testimonials from publishers who received the salt in payment for advertising, entitled to higher consideration and more confidence, than the certificates ordinarily attached to advertised medicines."

The *Transcript* follows the list with—

"We concur in the views of the *Traveller*, that these editorial verdicts, under the circumstances, are entitled to more than usual consideration."

It is worth while to say, that these two papers are edited by gentlemen, who, from their position in society, are supposed to know the weight that is attached to an editorial puff. It is to be supposed that they have some knowledge of the history of quack medicines, from the Royal Touch, through Weapon Ointment, Tar Water, Gordak's Drops, Swaim's Panacea, Brandreth's Pills, &c., down to Antiphlogistic Salt. Yet they publish, not as an advertise-

ment, but as editorial; not the *statement* of an irregular member of the medical profession, but as their own unbiassed *opinion*, a third of a column of the most decided recommendations of a medicine, of whose composition they know absolutely nothing.

The Antiphlogistic Salt is a solid medicine, easily susceptible of analysis, by the aid of the microscope or the test tube, or by both together. Let us see what it is.

I obtained a box of it, containing perhaps two drachms, more or less. It was not bought of any druggist, who could have an object in imitating it, but was obtained directly from Dr. Coggswell himself, was delivered from his own office and by his own hand. It is worth while to say this, lest the purity of the specimen should be questioned. The appearance was that of coarsely powdered borax, which I supposed it to be, slightly colored with some vegetable coloring matter, and scented with orris root. The box was covered with tin foil and wrapped in a printed advertisement, containing the directions for use. It is worth while to say this also, because, as soon as its composition is made public, the salt may change its character; and it is equally possible that some State assayer may be called upon to test a box *marked* "Antiphlogistic Salt," for the purpose of testifying that *that* box does not contain what my box did contain.

The Antiphlogistic Salt, thus obtained, was made up of the commercial bicarbonate of potash, in coarse powder, orris root, some vegetable coloring matter *not* a salt, and—nothing else. The only salt in the box was the bicarbonate of potash.

Bicarbonate of potash has been used from time immemorial as an antacid in dyspepsia, as a diuretic, and in some cases of inflammatory disease as an antiphlogistic. It has sometimes been used to such an extent and for so long a time as to deteriorate the quality of the blood, producing a state of the system akin to scurvy.

For two dollars a box you can get it of Dr. Coggswell; for a few cents a pound you can buy it of any grocer; and if the editors of the newspapers, who puff it, desire to cure themselves of any of the formidable diseases, the list of which is given above, they have only to request their wives to step into the kitchen and bring up the *sal æratus* box. B.

London Fever Hospital.—The income of the past year has been £132, 14*s*. 3*d*. less than the expenditure. The total admissions during the year had been 1761, in addition to the 121 remaining at the close of the preceding year, the admissions exceeding the latter by 735. The cases comprised 1061 typhus, 149 typhoid, and 183 scarlet. Out of the whole number, 1483 recovered, 289 died, and 94 remained under treatment.—*London Lancet.*

Reports of Medical Societies.

EXTRACTS FROM THE RECORDS OF THE SUFFOLK DISTRICT MEDICAL SOCIETY.
L. PARKS, JR., M.D., SECRETARY.

Dysphagia.—Dr. JEFFRIES read, from a letter addressed to him by a missionary in Asia Minor, the following description of a case of *dysphagia*, the writer—who was a physician as well as a clergyman—being the subject of the affection.

"My age is 32 years. Temperament sanguine. General health very good. Constitution naturally strong. The local difficulty from which I am suffering commenced about fifteen years ago; it consists essentially in a difficulty of deglutition, in respect both to solids and liquids.

"The phenomena attendant upon this trouble may be best illustrated by a statement of what occurs whenever I take my ordinary meals. When I commence eating, and for a little time after that I have begun to take food, I swallow apparently as well as any one else. I experience no difficulty at all; but, ordinarily, before I have eaten one fourth part of a meal, I begin to feel a sense of obstruction—a sort of constriction, apparently—in the œsophagus, as it seems to me. As I continue eating, this sense of constriction gradually increases. It does not ever amount to pain, sharp or dull, but is rather a feeling of constriction, accompanied with considerable dyspnœa, and an inability, or seeming inability, to speak. This sense of suffocation becomes insupportable if I continue to eat, and relief is obtained in only one of two methods—either after considerable voluntary effort a regurgitation of the food just taken is effected, or, a large quantity of fluid, ordinarily water, is drank at once, and thus the obstruction is removed, and immediate relief follows. In this drinking, the first and second glasses of water only seem to increase the feeling of constriction, and it is not until after the contents of a third or fourth glass are poured hastily down that the obstruction is removed and relief obtained. The water seems to act mechanically by forcing the food into the stomach. This once well accomplished, I feel just as well as when I first began to eat.

"Then I commence eating a second time; again experience the same difficulty after having taken about the same quantity of food; and relieve the obstruction as before, namely, by forcing down, as fast as I can, two, three, and not seldom four ordinary-sized glasses of water. Usually, I eat and "drink" thus twice at each meal. Sometimes, from some inexplicable cause, I am unable to remove the obstruction, even though I drink a *very* large quantity of water, say five or six glasses. Then the distress is much aggravated; the dyspnœa is much increased, and I wheeze like a man suffering from an attack of asthma. This continues until, in some way, either by putting my finger in my pharynx, or by a voluntary muscular exertion, I am able to effect a regurgitation of the food taken.

"All kinds of food, liquid and solid, meet the same difficulty of deglutition. Highly-seasoned dishes, and those of a more *liquid* character, are swallowed with more difficulty—that is, I am sooner obliged to drink—than unseasoned and more *solid* and dry kinds of food. In drinking simple cold water or any other fluid, I experience the same dif-

ficulty as in eating solid food. When I have drank one glass of water, I am obliged to drink at once a second and third glass in order to remove the sense of constriction produced by the first. When I commence eating, as I have said, I can take a *small* quantity of food before experiencing any sense of obstruction; but suppose that after having taken this small quantity of food I were then to stop eating, and not *drink* at all, the result would be that in about fifteen to thirty minutes I should have a feeling of uneasiness in my stomach, or œsophagus, which would soon be followed by a frequently-repeated *regurgitation* of the small quantity of food which had been taken, accompanied with more or less of a fluid secretion. I tried at one time this experiment of eating but a *little*, and not drinking for three days, and came near dying of inanition.

"Sometimes an indigestible article of food, after having remained in my stomach (or somewhere else) for twelve or fifteen hours, will, all at once, come up into my mouth, unchanged in taste or appearance, except having become, it may be, a little softened by the macerating process it has undergone. Oftentimes at night, just as I begin to drowse, a spasm occurs in my stomach, or œsophagus, which throws up a portion of liquid food with such violence as to fill my mouth, and even to pass into the posterior nares, and come out at my nose. Frequently during the latter part of the night, while asleep, I am attacked with dyspnœa and violent spasmodic coughing; so that I am obliged to rise from my bed and regurgitate a quantity, more or less, of partially-digested food mixed with a tasteless water-like fluid. In the morning, soon after rising, a considerable quantity of such fluid is sometimes thrown up. Occasionally, though seldom, I have an attack of proper pyrosis. My appetite is very good, and I very seldom suffer from any of the ordinary symptoms of dyspepsia or indigestion; seldom am troubled with either diarrhœa or constipation.

"This difficulty of deglutition began very gradually, and has continued slowly and almost imperceptibly to increase up to the present date. At one time a glass two thirds full of water occasioned as much relief as I now derive from drinking three or four. At no time has pain or a sense of soreness been produced by pressure or percussion of the epigastric region. At one time a common-sized ivory-tipped probang was passed without the least difficulty quite into the stomach.

"The chief remedial agents that have been tried, are the oxide of zinc, prussic acid, and electro-galvanism; none of which seemed to produce any effect.

"Is this case probably curable? If not curable, will it probably *yield at all* to medication? If so, to what extent? What is the *nature* of this disorder, what its prognosis, what its treatment, and how long should medication, if tried at all, be persevered in?"

Dr. Hodges referred to a similar case, previously reported by himself at another Society. The patient, aged 36, dated back his dysphagia to the earliest period within his recollection; and said that physicians whom he had consulted previously to Dr. Hodges, had pronounced the affection owing to hour-glass contraction of the stomach.

Dr. Jeffries remarked that in the case presented by him, the questions in point were, why does the food not pass? why liquid relieves

the obstruction? and what is the remedy? Dr. J. entertained doubts if the probang had ever really passed into the stomach.

Dr. GOULD thought the lesion to be most probably a diverticulum. The regurgitation of the food, during sleep, made against the theory of the affection being hysterical. The regurgitation, too, is shown to be from above the stomach, because the food ejected is found not to be acted upon by the gastric juice. If the case were one simply of hour-glass contraction, then the food would show the effects of the digestive process, because it is only necessary that a small portion of the stomach should be healthy, to enable digestion to take place. Dr. G. instanced a case lately reported by him, in which no more of the stomach was sound than could be covered by the palm of the hand; and yet digestion was perfectly well performed.

Dr. J. B. S. JACKSON remarked that diverticulum from the œsophagus was a rare lesion. He had, however, seen a case in which there was stricture of the œsophagus (like that of the urethra), with a dilatation above it, large enough to admit two fists.

It being suggested that the force with which the food was thrown up, in this case, would seem to indicate a more powerful muscle than the œsophagus, as the cause of the violent ejection, Dr. Jeffries replied, that we do in fact see the food thrown out with immense force, in common stricture of the œsophagus. Dr. Jeffries considered the case as non-malignant. That it was dilatation of the œsophagus from stricture of the cardiac orifice or of the gula quite near to the stomach. That the enlargement might be rather in the form of a diverticulum than a general and equal dilatation of the whole œsophagus. That the prognosis was unfavorable; and the treatment could be only palliative, being entirely mechanical.

Belladonna and Scarlatina.—Dr. BUCK mentioned a case, in which scarlatina had proved fatal, notwithstanding the previous use of belladonna. The patient was his grandchild. The father of the child—an apothecary—had desired to have the prophylactic power of belladonna tried upon his children; and Dr. Buck, though having no faith in the drug as a preventive, allowed it to be used. Both children were put upon it; the eldest (who subsequently had the disease and died) took 20 drops, three times daily, of a solution of one grain of belladonna in an ounce of water, for the space of two months; at the end of which time she was attacked with scarlatina. The eruption was very full, and the throat profoundly affected. The sister—an infant—though constantly with the affected child, has not yet had the disease—fifteen days having elapsed, since the death of the elder one. Dr. Buck mentioned the case, as bearing upon the prophylaxis of scarlatina; and also as confirming, so far as it goes, his disbelief in the contagion of the disease.

Dr. BUCKINGHAM criticised the dose of belladonna given in this case, as being large, in view of the long-continued employment of the drug, and of its cumulative effect.

Criminal Abortion.—Dr. H. R. STORER offered a resolution "That a Committee be appointed to consider whether any further legislation is necessary in this Commonwealth, on the subject of *criminal abortion*, and to report to the Society such other means as may seem necessary for the suppression of this abominable, unnatural, and yet common

crime. Said Committee to consist of three, and to be appointed by the Chair."

Dr. Storer, at the suggestion of Dr. Jeffries, added this amendment:—"And that said report, when accepted by this Society, shall by it be recommended to the Massachusetts Medical Society as a basis for its further action."

Dr. Storer prefaced the resolution with the following remarks:—

"He desired to bring before the Society a subject which imperatively demanded its early and decisive action. Somewhat over a year ago, the present professor of obstetrics in the University (Dr. Storer, Sen.) had called attention, in a public inaugural address, to the alarming increase of *criminal abortion* in this community, and to the fact that the initiatory steps towards suppressing the crime should come from physicians. When the address alluded to was subsequently published, so much of it as bore upon this question, as also upon a kindred one, the prevention of pregnancy, was suppressed—in deference to the request of other gentlemen of the College Faculty, but entirely against the author's will. That gentleman had since been repeatedly called upon for a reiteration of his views; many months had, however, now elapsed, and as there seemed little or no probability of such being done at present, if at all, his son, after duly ascertaining this fact, had no hesitation in at once bringing the subject before the Society; it being acknowledged by all, in the least degree conversant with this matter, that immediate action was necessary.

Dr. Storer quoted statistics from a recent memoir by Tardieu of Paris, in the *Annales d'Hygiène Publique et de Méd. Légale*, for 1856, showing how common was the crime in this country as compared with others; so common indeed as to have led foreigners to suppose that the procuring criminal abortion was with us an ordinary and well-established branch of industry, not interfered with by the law; as indeed, to all intents and purposes, is at present the case.

Dr. S. referred to our statutes on this subject, and to the ignorance prevalent in the community respecting the actual and separate existence of fœtal life in the early months of pregnancy. He dwelt on the moral and absolute guilt of the parties offending, and on the necessity of prompt and efficient action by the profession, and called upon the Society, as representing the physicians of Boston, to take such steps as would alike further ensure the innocence in this matter of all its members, and show to the community the sincere abhorrence with which they viewed the crime."

In conclusion, Dr. Storer quoted from an eloquent editorial article in the Boston Medical and Surgical Journal of Dec. 13th, 1855.

Dr. Storer, Sen., expressed his satisfaction that the subject was at last to be brought before the community. He held without abatement the views he had formerly expressed regarding it; the crime, if it existed to the extent all would allow it did exist, should be repressed, and it was the duty of physicians to expose and to denounce it. Several years since, the profession in Boston had insisted that the office of coroner, so great were the abuses connected with the system as then existing, should be filled by medical men; in consequence of their action, the change had been made. There could be no doubt that similar action now, on the part of this Society, would be followed by a similar result. He disclaimed any collusion with his son in thus

bringing up the matter, but was delighted it had been done, and had no doubt of the ultimate result.

On motion of Dr. Geo. S. Jones, the rules were suspended, in order that the Society might take action upon Dr. Storer's resolution, which, with the amendment, was unanimously passed.

The Chair appointed upon the committee, Drs. H. R. Storer, Bowditch, and Ellis.

Circumcision.—Dr. H. R. Storer stated that he had been called in the night to a Jewish child, who was suffering from alarming hæmorrhage from the operation of circumcision. The Jewish operator—an expert—had tried to stop the bleeding, without success. Dr. Storer, by thorough cauterization with nitrate of silver, stopped the hæmorrhage.

Lesion of Back.—Dr. John Ware described certain cases of lumbago—so called—coming on suddenly after exertion. They resemble, he said, the sudden rupture of a muscle in the calf of the leg, in which the first sensation is that of being struck a blow, as by a stone. Dr. Ware considered these phenomena (i. e., the lesion of the back and that of the leg), of the same nature. The first-mentioned affection is usually about the small of the back—confines the patient to the bed from three to four days—leaving behind some soreness. For a time, large doses of opium are usually required.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, MAY 7, 1857.

CONTRIBUTIONS TO OUR PAGES.

We have more than once alluded to the paucity of communications from practitioners in our city and its immediate vicinity, and perseveringly asked "for more," like Oliver Twist at Dotheboys Hall. Nearly all the articles offered us come from a distance; and whilst we fully acknowledge the interest and value attaching to most of them, we cannot but feel an increasing desire that the numerous sources nearer home should be oftener unstopped. Outside of the reports of our Medical Societies, our city brethren but very infrequently send us a paper, *unsolicited*. We some time since expressed our intention of making requisition here and there for aid, and have carried our resolve into effect with some success in the way of obtaining bibliographical notices. To those gentlemen who have from time to time sent us original communications and reports of cases, we renew our former hearty thanks. Their kindness, whilst it will not allow us to *tease* them for other articles, encourages us to *hope* for them; and we point to their example as one we look daily (alas! how long?) to see followed.

When we observe long and elaborate communications in our own, and in foreign journals, many of which are not paid for, although doubtless the majority are—we cannot but ask whether more of our medical friends here cannot find leisure, *if they will*, to help us on our way. Perhaps, were it possible for us to devote more time to our labors in this line, we should not need to be so clamorous for assistance,

although even then much would necessarily be demanded. We are sure, however, that when it is considered how much journalizing trenches upon the time of practitioners, our occasional calls will not be deemed unreasonable.

We referred to articles furnished to medical journals at home and abroad. In the *Edinburgh Medical Journal* for March, 1857, Professor J. Hughes Bennett contributes a most valuable article of 27 pages. Succeeding this, is another by Professor Duncan, of 7 pages; one by Dr. Baikie, of 12 pages; one by Benjamin Bell, Fellow of the College of Surgeons, of over 4 pages. These gentlemen are all of Edinburgh, and high in repute, as is well known. We took up this particular Journal, because it happened to be lying on our table; and its pages thus amply manifest the *local* interest shown in its success.* It is a monthly journal, and consequently admits of longer articles than a weekly. But there is far more pressing necessity that the latter should not be left with a "beggarly array of empty boxes" for its editors to gaze at, in despair for their "first article," or their "long primer."

We gratefully acknowledged a goodly supply of papers from a distance—most of them of much value. There are some, however, which come to us in the most extraordinary guise, and we must protest against manuscripts whose matter, how excellent soever it be, must be *translated* before any compositor can set it. We prefer to select, for our *translations*, from foreign journals. In addition to the frequent necessity of copying "awful" manuscript, it is a very common thing for us to be obliged to spend half an hour, or even an hour, in writing out, in full, the words answering to the most barbarous and unwarrantable abbreviations of Latin and other terms—as in prescriptions and formulæ. Sometimes, if sundry of these shortened, or rather *mangled* words escape our correction, we are called upon by some reasonably puzzled correspondent to know *what* such or such a medicinal ingredient may be—no wonder they do not discover! It often costs us a deal of time to decipher them in the original *cacography*. We trust those who expect to appear in print will not impose this double labor upon us any longer, for we assure them we have full enough to do without it.

May we not hope, as our Journal is an old settler, and has thereby a strong claim upon its immediately-surrounding friends, that they will not let it want the *pabulum* they can so easily supply? Will not such friends, both old and young, give a *local* interest and impulse to it, which will bear favorable comparison with that so observable in many other places, where, to say the least, there are no greater opportunities for observation, nor any more facilities for communication?

DR. NEWMAN'S CASE OF LABOR.

Early in the month of December last, Dr. A. Newman, then, or just previously, of Attleborough, Mass., reported in our pages a case of puerperal peritonitis; the patient being attended by himself and other friends in conjunction. During the progress of the case, Dr. E. Sanford, an homœopathic practitioner of the same town, was called in.

* We mention only the main communications. The reviews and society reports are full, also.

We appended a note to the account, expressing an opinion, legitimately founded upon the venerable axiom that "too many cooks spoil the broth," at which much umbrage has been taken *now*, after the above-named lapse of time. Without citing the exceptionable language in which Dr. Sanford demands a hearing, we give his facts as stated by himself, with some abbreviation, but without suppression of anything essential.

Dr. Sanford writes: "On Tuesday, October 15th, the patient was delivered by Dr. Newman, and treated by him in conjunction with Drs. Clapp and Bronson, until the succeeding Saturday afternoon, at which time the case came into my hands. Her physicians had distinctly and without any reservation declared that the patient could not recover. Dr. N. stated to several persons, who can attest the fact, that she would not survive twelve hours longer. The case was certainly sufficiently severe." Dr. S. goes on to enumerate the symptoms, viz.: excessive abdominal distension; acute pain; intolerance of the lightest touch; suppression of urine; and the concomitant fever. Although none other than an unfavorable prognosis was admissible, Dr. Sanford considers that his treatment induced "a partial amendment." There was, he says, an increase of the quantity of urine, and a diminution of the abdominal tension and sensitiveness. The thirst abated; the skin and tongue became more natural, and the patient was every way more comfortable and her appearance better. "Her former attendant lingered about the sick room, ostensibly as a friend interested in observing the course of the disease. Without my knowledge, and unknown to me till after the decease of the patient, he administered to her a dose of morphia, on the third or fourth day of my treatment. Presently the favorable tendency of the case disappeared; and death resulted, six and a half days after her former attendants had pronounced her continuance, for twelve hours longer, impossible. This fact is kept entirely out of view in the article reporting the case; and it is carefully represented that, at the time Dr. N. was dismissed, the patient, though quite sick, was expected to recover. That the patient showed a mitigation in the severity of her symptoms after coming under homœopathic treatment, is a fact beyond dispute; and that the promised improvement was arrested after the surreptitious administration of morphia, is capable of proof."

We omit the last two paragraphs of Dr. Sanford's letter, because we do not wish to make our pages a battle-field, nor a medium for the interchange of recrimination and personal differences.

PALMER'S PATENT LEG.

We have before called attention to this admirable invention, which has now stood the test of many years' trial, and still maintains its right to be considered the best artificial limb. It appears to combine all the requisites of a substitute for the natural member; lightness, strength, flexibility and simplicity. Many patients begin to walk directly after they have been fitted with one of these legs, and it would not be easy to detect the wearer among a number of other persons, in a considerable number of instances. We are glad to notice that Messrs. Palmer & Co. have removed their manufactory from Springfield to this city, No. 19 Green street, where they will have every facility for carrying on the business in the most extensive manner. We recommend to all

who have been so unfortunate as to lose a limb, to call on Messrs Palmer & Co., where they will find the best means of supplying their loss. The removal of the establishment to Boston will greatly add to the convenience of those from the Eastward and the Provinces who require artificial limbs, and who hitherto have been obliged to travel some distance to reach the establishment at Springfield.

The late Dr. Harrison, of Middletown, Ct.—At the annual meeting of the Middlesex County (Ct.) Medical Society, held in Haddam on the 23d ult., the following resolutions were passed by a unamimous vote:—

Resolved by this meeting, That we recognize the afflictive hand of God in removing by death, our late fellow member, the lamented DAVID HARRISON, M D., of Middletown, and that we tender to the surviving relatives and friends our sympathy in their bereavement, and our tribute of respect for the character and professional worth of the deceased.

Resolved, That we cherish his memory as an able, efficient and assiduous member of the profession.

Resolved, That these resolutions be signed by the chairman and clerk of this meeting, and published in the Middletown and New Haven papers, and in the Boston Medical and Surgical Journal. ASA M. HOLT, Chairman.

ELISHA B. NYE, Clerk.

Artist to the Massachusetts General Hospital.—We take pleasure in mentioning the appointment of Lucius M. Sargent, Jr., M.D., to the post of artist to the Hospital. Dr. Sargent is eminently fitted for this office, and his accurate knowledge and skilful pencil will prove of inestimable value to the institution and to the profession generally. It is often of the highest importance that drawings should be taken of specimens *in* or *ex situ*, and no hospital should be without its artist.

IN the Parish of St. George's, London, according to Mr. Jay, the vaccinator of the Parish, there has not been a fatal case of smallpox for more than a year and a half—the population being 20,000.

Health of the City.—Scarlatina is again slightly on the increase, 12 deaths having been reported during the last week, being the largest number from any one cause except consumption. The total number of deaths for the corresponding week of last year was 67, of which 8 were from consumption, 7 from pneumonia and 3 from scarlatina.

ERRATUM.—In the Journal for April 16, page 228, the name of Prof. L. S. Joynes, of the Medical College of Virginia, in Richmond, was unintentionally printed "Prof. Jones."

MARRIED,—In Worcester, 1st inst., S. M. Bigelow, M.D., of Paris, France, to Miss Lucy Barton.

Communications Received.—Oxide of Zinc for Profuse Sweats.—Case of Tumor of the Parotid.

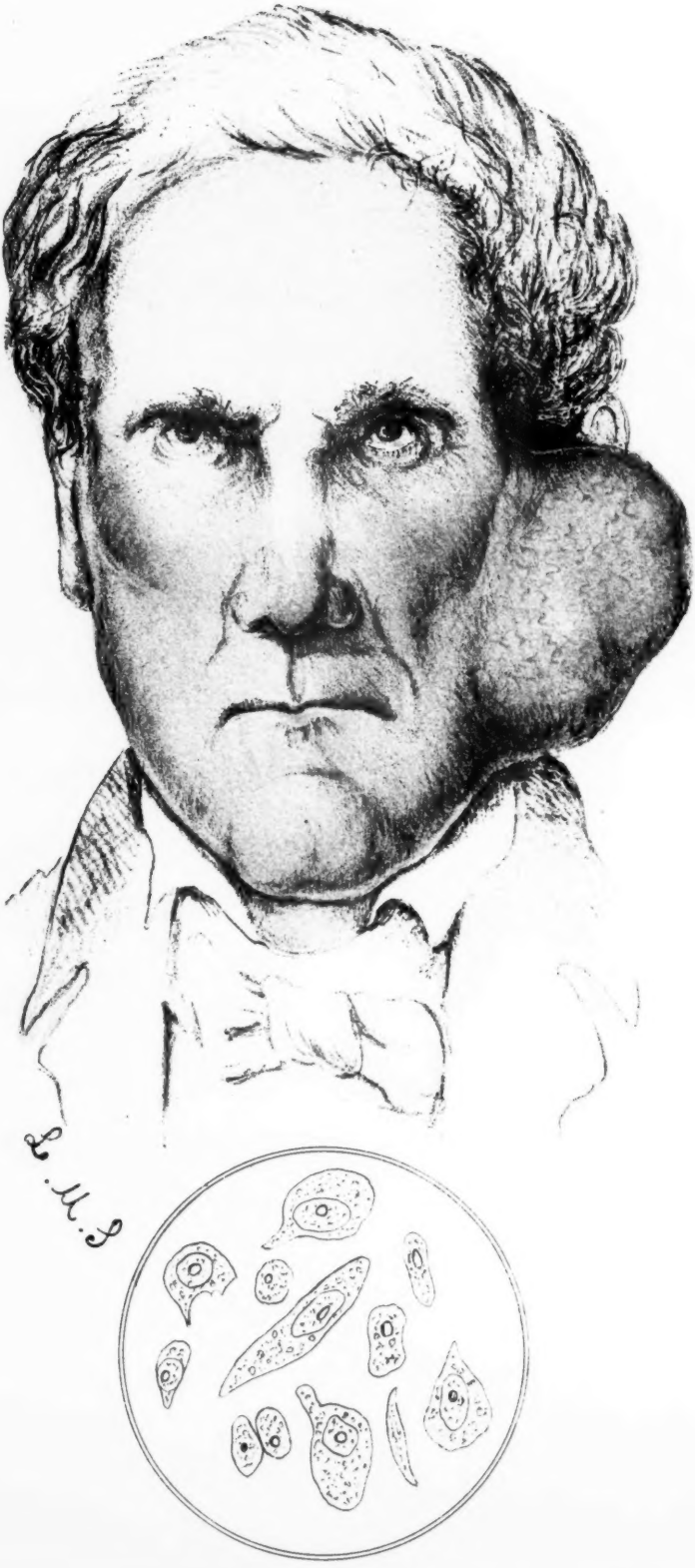
Deaths in Boston for the week ending Saturday noon, May 2d, 72. Males, 30—Females, 42—Accident, 1—apoplexy, 1—inflammation of the bowels, 1—bronchitis, 2—inflammation of the brain, 4—softening of the brain, 1—consumption, 18—convulsions, 1—croup, 3—dropsy, 1—dropsy in head, 4—infantile diseases, 3—puerperal, 1—erysipelas, 1—typhus fever, 1—typhoid fever, 1—scarlet fever, 12—gangrene, 1—homicide, 1—disease of the heart, 2—inflammation of the lungs, 1—congestion of the lungs, 1—disease of the liver, 1—old age, 2—pleurisy, 2—syphilis, 1—teething, 3—unknown, 1.

Under 5 years, 33—between 5 and 20 years, 3—between 20 and 40 years, 18—between 40 and 60 years, 10—above 60 years, 8. Born in the United States, 52—Ireland, 18—other places, 2.

The Blind in New York.—The Institution for the Blind in New York contains 175 pupils. The last report avers that the blind, as a class, show languid health, occasioned by constitutional tendencies which caused their blindness, and aggravated by the difficulty of keeping up habits of bodily activity. The report reviews, says the *New York Times*, the disadvantages which attend public displays of the blind. The aim of the institution is "to impart instruction which will be of real service in after life," and with this view of conducting it on truly parental principles, exhibitions of the pupils are no longer considered necessary. "The blood of youth," says the report, "should not be distempered by the unhealthy excitement of public life, with its false standards of happiness, its illusions and disappointments; nothing can be more unphilosophical than the notion that excitements natural to maturer years, can add to the mental activity of the young." The mechanical instruction of the blind proceeds as usual. The expenses in this department, in 1856, were $17,000. Instruction cost, $985. The musical classes are conducted as usual, effectively.

Arsenic instead of Magnesia.—A mistake of a distressing and fatal character occurred recently in Baltimore. Mrs. Margaret Macfarland, being of a plethoric habit, says the *Sun* of that city, accustomed herself to taking magnesia and like aperients for the purpose of preventing a rush of blood to the head. One night, feeling unwell, she was advised by a female friend to take some magnesia, and she accordingly sent to the house of a neighbor to obtain some. The supposed magnesia was procured, and she swallowed it. For a time she felt quite comfortable, but at midnight she was seized with vomiting and severe cramp in the stomach. Dr. O'Donnell was called, and he pronounced the symptoms to be those of poison. Everything possible was done for her, but she died shortly after in great agony. The bottle from which her neighbor had taken the medicine proved to contain arsenic, there being a bottle of the same description containing magnesia, and thus the mistake occurred.

Amylene.—We have tried this new anæsthetic in two cases at the Commercial Hospital, but were unable to render the patients insensible. In the first case we used but about half an ounce—all that we could procure—in the second about an ounce. It produced no irritation of the glottis, but the odor was rather unpleasant. We doubt whether it will ever take the place of chloroform or sulphuric ether.—*Western Lancet.*

Completely Impermeable Stricture.—On the 17th of March, Mr. Cock submitted a patient at Guy's to treatment who had an impermeable stricture of the urethra of many years' duration, with numerous false passages and perineal fistulæ. A great deal of the spongy portion of the urethra was a shrivelled-up, impermeable cord, through which not a drop of urine had passed for months. With this Mr. Cock for the present did not interfere. He, however, performed perineal section, and divided that part of the urethra in front of the prostate, and succeeded in passing an instrument into the bladder, accomplishing this by passing his finger into the rectum, and hitting the urethra anterior to the prostate. His urine will be passed per perinæum still, and it will be a long time before anything can be done with the other portion of the canal, which will no doubt, to some extent, improve. He has suffered from infiltration of urine and its consequences. Mr. Cock knows of cases where no urine has passed through the penis for years, the patient being obliged to sit down to perform urination.—*London Lancet.*

Glycerine and Borax in Cracked Tongue.—Dr. Brinton has under his care an inveterate cracked tongue, which (like that of the late Charles Mathews) had baffled all attempts at alleviation for many years. It could not be referred to any syphilitic poison, and rendered eating, and especially speaking, very painful. Dr. Brinton made use of a favorite remedy of his in such cases—viz., borax dissolved in a lotion of glycerine (Price's Patent Candle Company's) and water (two scruples, one ounce, and four ounces respectively). It at once gave marked relief; and after a few days, during which it was the only remedial agent, the improvement seemed increased by iodide of potassium and bark taken internally. The patient has now considered himself well, and discontinued the lotion for some weeks, and the cracks are only visible as depressions in the mucous membrane.—*Ibid.*